Biogeosciences Discuss., 12, C5418–C5421, 2015
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Biogeosciences
Discussions
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Interactive comment on "Millennial changes in North Atlantic oxygen concentrations" *by* B. A. A. Hoogakker et al.

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Received and published: 15 September 2015

The manuscript presents new data of millennial variations of dissolved oxygen in the North Atlantic. I think the manuscript is well written and illustrated and that the main conclusions are supported by evidence. Quantitative reconstructions of dissolved oxygen are rare but, in my opinion, extremely valuable. I applaud the authors for a job well done. The comments listed below are rather minor, but the authors may find them useful in case they prepare a revision.

Title Page: typo in first affiliation (Univeristy)

Fig. 1: is it necessary to show the whole globe in the top map? It may be better to zoom into the North Atlantic to see better which sections are used.

Page 12949, lines 16-17: I don't think that this statement is accurate. No references are provided that would support it. Contrary to what is claimed here Gregory et al. (2005, GRL 23, L12703) have argued that the AMOC reduction is mostly due to heat fluxes rather than freshwater fluxes.

Page 12950, line 2: You may also want to cite Schmittner & Lund (2014, Clim. Past 11, 135-152), who present the first evidence from distributed deep ocean d13C compared with model simulations.

Page 12950, lines 20-23: I think this is a very optimistic statement. I also don't see how those reconstructions would directly help constrain future projections. I'd suggest to remove the statement or to provide some arguments supporting it.

Page 12952, line 16: Here and elsewhere I suggest to replace d13C with d13C_DIC in order to differentiate between water column data from other sources such as carbonates.

Page 12952, lines 22-24: d13C_DIC distributions in the ocean are also affected by temperature dependent fractionation during air-sea gas exchange (e.g. Lynch-Stieglitz et al. BGC 9, 653-665) and the degree of equilibration of surface waters with the atmosphere (e.g. Schmittner et al. 2013, Biogeosciences 10, 5793-5816)

Page 12952, line 6: Fig. 3 is discussed before Fig. 2.

Page 12953, lines 13-14: The following part of the sentence is somewhat ambiguous: "inferred from bottom water and anoxic boundary dwelling foraminifera" because it is not clear if the forams are dwelling in bottom water and in the anoxic boundary, or if "inferred from bottom water" means inferred from insitu measurements of bottom water oxygen concentrations. Please clarify.

Fig. 4: it may be interesting to plot the cibicides and Globobulimina d13C separately to see which of those dominate the resulting variability of the gradient.

Page 12956, lines 8-10: Please show the d13C so that the reader can understand this

note.

Page 12956, line 12: “event” should be “events”. Also, please note C21 in the figure.

Fig. 4: what is the event (grey bar) between H6 and C19 in the lower panel?

Fig. 5 and discussion: it seems to me that the millennial events are barely resolved. H1, e.g., seems to be two data points. H4 also two or three (hard to see from the figure). I wonder if bioturbation could dampen the signal. I suggest to discuss this point, which may also be relevant for the model-data comparison on page 12960.

Page 12957, lines 1-2: “they are significantly reduced compared with warm interstadial intervals as well as the LGM.” They don’t seem to me to be significantly reduced compared with the LGM. Please provide statistical calculations such as means, errors for the means and significance levels for the means to be different.

Page 12959, lines 6-8: Note that the first modeling study to show this was in fact Schmittner (2005, Nature). I suggest to cite the original paper.

Page 12960, line 23: “smoothed out” by bioturbation? Another reason for the larger amplitude changes simulated by the Schmittner et al. (2007) model is that their simulation starts from a pre-industrial background state. If a glacial state with a weaker and shallower AMOC was used the amplitude of the oxygen changes at the deep site would have been presumably smaller. This may also explain the overestimated amplitude in benthic d13C simulated in the North Atlantic by Schmittner and Lund (2014).

Page 12960, lines 19-20: How were the ranges of 24-60 uM (intermediate) and 15-101 uM (deep) determined? Please explain in detail how those numbers were calculated. Perhaps a table with means and error estimates for each of the events may be useful. From Fig. 5 it seems to me that many interstadial events are above the 235 threshold where the method becomes non-quantitative. So, how exactly were the pre-Heinrich stadial reference values calculated? And how exactly were the Heinrich values calculated.

Page 12961, lines 11-13: “For North Atlantic Intermediate Water however there is now mounting evidence that this overturning cell was stronger during millennial cool events.” I don’t agree with this statement. I don’t think the intermediate cell was stronger during stadials than during interstadials. I have looked at some of the references provided on page 12958 to support that notion, but I’m not convinced. I think we should be careful in interpreting the radiogenic isotopes. Some of the issues have recently been noted by Hayes et al. (2015, DSR II 116, 29-41).

Supplementary Information: Too little information is provided on how the modern water column data were obtained, processed, and analyzed. Latitude, longitude and cruise information is missing. From the general website provided in the Figure caption to Fig. 2 it is not possible to reproduce the dataset. Please provide detailed steps there were taken and analysis. Are the water column d13C data quality controlled? An alternative global dataset with qualitiy controled data is available here: http://cdiac.ornl.gov/oceans/Schmittner13bg.html

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Interactive comment on Biogeosciences Discuss., 12, 12947, 2015.